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ON CHORISIS.

AS AN EXPLANATION OF CERTAIN VEGETABLE STRUCTURES.

BY THE REV. WILLIAM HINCKS, F.L.S., F.B.S. EDIN.,

HON. MEMBER OF THE LIVERPOOL LIT. AND PHIL. SOC., YORKSH. PHIL. SOC., BOT. SOC.
OF CANADA, CORRESPONDING MEMBER OF THE ESSEX CO. INSTITUTE AND THE
BUFFALO NAT. HIST. SOC., PROFESSOR OF NAT. HIST. UNIV. COLL., TORONTO.

It has of late years, been the aim of philosophical botanists to establish a few general laws of vegetable structure, capable of explaining all the phenomena which fall under our observation, so as to exhibit a common plan in all the various forms of plants, and show the kind of variations from the general type which occur in each particular instance, or in other words, to trace to the action of intelligible causes the peculiarities observable in each distinct structure, so as to show what is common to many, and how mutual relations are manifested in the midst of apparent diversities. This is, perhaps, to be accounted the highest and most interesting part of the study of nature, and if it must necessarily be preceded by the examination of the details of individual structures, always varied, curious, and attractive, it at least arises out of them as naturally as the philosophy of every science arises out of its facts and observations, combined and meditated upon by the highest intelligences amongst its votaries, aided at times by the happy thoughts of humbler labourers in the same field. I design now

to bring under consideration one of the principles which has been proposed as a general expression of a number of facts in the structure of flowers, or, as a cause which may be assigned in explanation of some remarkable features belonging to particular flowers, explaining at once the relation to the common plan, and the meaning of the apparent discrepancy in the special case.

My subject is what was, I believe, first named by the French botanist Dunal, *chorisis*, a Greek word expressing *division* or *separation* and applied to supposed cases of a single organ in a floral circle being, so to speak, resolved by subdivision into a number of parts. At present, whilst many high authorities admit this principle as affording the true explanation of some remarkable facts in the structure of certain flowers, other authorities of not less general weight entirely reject the principle as unsupported by any sufficient evidence, and not needed to explain the phenomena. In such a case any contribution towards determining the point in dispute may be received with patience and may have some claim to attention. It may be expedient in the first place to consider what are the principles in relation to the structure and variation of flowers which may be regarded as known and established, and to what extent they go in explaining the appearances before us that we may be prepared to judge how far further assistance is required, and, if so, how far the proposed principle supplies what is wanted: nor will this view of what may be said to have been accomplished in an important field of enquiry be in itself destitute of utility since comparatively few years have changed the whole aspect of botanical science, and our greatest practical botanists continue to employ in description, terms founded on erroneous opinions, and suggesting false views where on so many accounts the utmost correctness of language is demanded, besides that the truths to be enumerated, though well established and admitted by those esteemed the best judges, are by no means so generally received and applied as not to require to be explained and enforced.

The 1st principle to be noticed is that every flower originates in an ordinary bud modified in its development, the increase of the axis being checked and the leaves reduced into circles and made to assume the characteristic forms of floral organs, which setting aside intermediate and anomalous ones are 4, described and named as follows: the exterior one, usually retaining most of the leafy character, called as a whole the *calyx*, and its separate organs named *sepals*: within it an-

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other set of protective or enveloping parts, usually of a more delicate texture, and more likely to be colored, called the *corolla*, and its parts *petals*; then a set of organs so transformed as for the midrid to become a simple support called the *filament*, the lateral expansion to be contracted into cells forming the *anther*, whilst the superficial cells of its infolded surface are specialised into sperm cells called *pollen*. These organs as a whole are called *androecium* and singly *stamens*. In the remaining circle the leaves are made to bear on their margin or at their base germ cells called *ovules*, this expanded portion of the leaf or of several such leaves united being the *ovarium*; the apical portion generally drawn out to some length, is the *style*, and the naked glandular tip is the *stigma*. The whole circle of these leaves is the *gynoecium*, individually they are *carpels*. As there are four distinct modifications of leafy organs, forming in typical examples as many circles, there is a manifest convenience in having a name for each circle as a whole and for the parts of each, besides any names required to designate special portions of each organ. I have adopted names from good authority using care in their selection. The chief thing to be observed is the use of the term *gynoecium* for the whole of the inner circle and *carpel* for each separate part. I have judged it necessary to reject entirely the Linnaean term *pistil*, because, the true theory of the structure of the flower not being then understood, he used the term, sometimes for the whole circle of carpels when so united as to seem a single organ; sometimes for each separate style where the ovarian portions of the carpels are united, but their styles distinct, and sometimes for each carpel where they remained entirely separate, the word is useful enough in reference to the Linnaean artificial system, but cannot be employed to express what is now known without being a source of confusion. It is much to be regretted that eminent teachers of the science will persevere in employing it, especially as the evil is greatly aggravated by attempts to give the term a new meaning or to persuade us that Linnæus employed it in accordance with our modern ideas.

2. Having considered what seems well established, respecting the origin of the flower and the nature of its parts, what first claims our notice is the variation in the number of circles.

We have mentioned four differing in kind, but we may have one, two, or three of these absent, and we may have them increased by the occurrence of many circles of one kind of organ. The difference is in the development of the axis of the flower, which varies from a single

circle to an indefinite number, the increase being chiefly in the inner ones. Whenever a flower presents a crowd of similar organs, whether manifestly in successive circles, or by their closeness thrown into a confused mass, the explanation which first occurs to the botanist is multiplication of the circles, whether there may be sometimes reasons for rejecting this and seeking another may be afterwards considered.

3. Our third principle relates to the position of the circles. The most natural and general is with the parts of each (the numbers conforming) alternate with those of the circles without and within it. This evidently depends on the same spiral plan of growth which produces the arrangement of leaves on a stem, the members of the successive circles being indeed produced in the same plane, but when some growth becomes necessary to obtain space for another circle, the advance of the axis being as usual spiral, and to a degree just sufficient to make the parts alternate, but besides that a whole circle may be so nearly suppressed by close pressure, as to be scarcely, if at all, perceptible, which would make those immediately within and without appear opposite, the alternation being maintained by the unnoticed intermediate circle, which is doubtless the true explanation of the stamens opposite to the petals in the Primrose family, it is quite conceivable that in certain cases the spiral course might be either prevented, or carried too far for alternation, the parts thus becoming opposite and abnormal examples occurring in which this is seen to take place, proves that we are justified in assuming it as a sufficient explanation of the rare instances in which adjoining circles with opposite parts occur. Dr. Lindley has justly appealed to varieties of *Camellia*, in which the petals are ranged in regular lines, giving the flower a star-like aspect as proof of the possibility of the opposite arrangement taking the place of the alternate, and those who think otherwise are driven to the most extravagant suppositions to evade the force of his argument. But I must afterwards recur to this subject in another connection. At present I wish to show the real nature of the law of alternation, and the possibility of deviation from it in exceptional cases, without disturbing our idea of the plan of structure or driving us to imagine other causes in operation.

4. The degree and mode of development of the separate leafy organs which form each circle may vary from the smallest to the fullest extent, and through several remarkable differences of form. All the parts of the flower consist of leaves modified in their development, and

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each is capable of assuming any of the functions, for we have monstrous examples (and I quote none but what I have seen) of carpels occurring among the exterior parts of a half-transformed bud, petals and imperfectly-formed stamens being found within; of stamens with anthers present having stigmas at their tips and imperfect ovaries at their lower portion; of petals and stamens passing by all degrees into each other and of all the circles returning to leaves. Besides these there are well-known intermediate conditions such as used to be called nectaries, and besides the expanded or unfolded condition of an organ, tubular, hooded, and spur or horn-like enlargements are not unfrequently met with. The leading effects of varying development may, in addition to what has been already pointed out, be conveniently noticed under the following heads, connection or separation of parts; equality or inequality of the parts of a circle, and influences on the number of parts. As to the first of these, it is a law of vegetable structure, that portions of growing plants, whether of the same, or of closely allied kinds, being in contact and continuing so, for a time without agitation, will form tissue so as to unite and become as one. This law prevails in the parts of flowers as elsewhere. The result is coherence when organs of the same circle unite by their edges, adherence when organs of adjoining circles unite by their surfaces. Increased development of the parts promotes coherence; closeness of the circles promotes adherence, and differences in these particulars have much to do with the variations of the common plan of flowers.

We need not, however, be in any doubt as to the true explanation of what occurs, as we are familiar with cases of degrees of coherence from the slight attachment of the petals of a Flax or Woodsorrel to the complete union of these parts in a Convolvulus or an Erica, from the connection of the petals at the base only in some cases, to its reaching the very tip in others, and we may have seen a little starvation restore a Bellflower or Convolvulus to five separate petals.

It is necessary, to be able to express what happens in precise and accurate language, and as the terms *monosepalous*, *monopetalous*, affirm what is well known not to be true, and are fitted to obscure the ideas of students, whilst DeCandolle's terms, *gamosepalous*, *gamopetalous*, are figurative and too long, and have met with little acceptance, I take this opportunity of proposing terms long used by me, as a teacher, which seem fully to supply what is needed without being liable to objection. Let the coherent parts be called *synsepalous*,

synpetalous, and if you please, *synandrous*, *syncarpellous*, whilst separation may be expressed by *aposepalous*, *apopetalous*, &c. Adherence arises from pressure of the circles on each other, or expansion of the torus or receptacle, so as to adhere sometimes outward on the lower part of the calyx, sometimes inward on the combined carpels, sometimes in connection with both, so as to place the fruit below the other circles of the flower and produce the epigynose structure—it readily explains many phenomena of common occurrence in flowers.

Regularity and irregularity of flowers depend entirely on the equal or unequal distribution of nutriment to the parts of the successive circles, the causes of which differences are often undiscernible, though the fact is certain. Sometimes the more developed parts are in all the circles on the same side of the flower; in other cases the opposite sides are enlarged alternately. In other instances their regularity is produced by an opposite pair being enlarged in each circle (where the whole number of parts is even), or by this arrangement being alternated in the successive circles. It must be evident how many modifications of flowers are explained by these considerations.

The primary law respecting number is found in the tendency to the number three in the circles of mono-cotyledonous plants, and to five in those of dicotyledonous plants. The first is an ultimate law of the organization of plants abundantly established by fact, but hardly capable of being connected, so far as we can at present see, with anything else we know of their nature. It may be doubted whether the second is not connected with the first in as much as one cotyledon or primordial leaf is found to imply a circle of three parts, two would therefore be expected to produce six, but this supposes the combination into one of two circles of three. Now we have other examples of this sort of combination of circles of parts exhibited to us by certain anomalous flowers, in sufficient number and variety of cases to suggest a sort of rule as to what is likely to happen, and from them we infer that in ordinary cases one part would be lost in the union. That under considerable pressure a part would be lost at each point of junction or two in the combined circle, whilst very close position, with circumstances unfavourable to development, such as give us occasional examples of two and one part in a monocotyledonous plant might occasion any of the lower numbers to occur in a dicotyledon. I found the explanation here given of the prevailing number of dicotyledonous plants on the careful examination of a considerable number of those monstrosities, not

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of very uncommon occurrence, in which two flowers are combined into one from their origin, owing to their buds having been adjacent. I can now distinctly recall examples in two or three species of *Iris*, and in at least three species of *Oenothera*, my cultivation at one period of numerous species of those genera affording me the opportunity of observing the anomalies to which they are liable. I had various instances of circles of five in the monster *Iris* and of seven in the *Oenothera*—one instance of four in the *Iris* in a single circle and one of only three, the exterior circles having five, and the tube showing sufficient marks of the union. In the *Oenotheras* observed, which embraced several species, there were uniformly seven parts in each circle, that is, seven sepals, seven petals, fourteen stamens and seven carpels. I gave some account of these monstrosities to the Linnæan Society in 1839, and it has since occurred to me that they establish a law respecting the combination of circles of growing parts, which may explain the tendency to the number five in Dicotyledonous plants, since, when growth is carried on from a single cotyledon, we find the number three in the circles, and where there are two cotyledons we might expect the circle to be double, but the fact of the loss of at least one part in combinations of two circles on the same plane shows why the number five takes the place of six. The liability of the natural numbers, five in Dicotyledonous and three in Monocotyledonous, to be reduced by mere pressure or by irregularity, is obvious from what has been already said. We find by observation that the number of parts in the successive circles of the flower is usually equal, but that the inner circle, being exposed to greater pressure, is apt to have fewer than the others—three and two carpels being very common in Dicotyledonous plants. In some structures the numbers in the different circles do not at all correspond, but this, which is characteristic of particular families, is less common, and its origin is one of the most obscure and dubious points in the theory of the flower. When parts are absent either from pressure or irregularity, we must remember that the fact is due to a special cause of abortion, not to the total absence of the part from the structure, and consequently that circumstances may occur from more abundant or equally distributed nourishment, which may in anomalous examples restore the missing part. Such examples are, indeed, almost needed to confirm our judgment as to the causes of the ordinary absence of these parts, and have therefore great interest for the philosophical botanist. In the natural family of the *Onagraceæ*, to which the genera *Fuchsia*

and *Oenothera* belong, the reduction by pressure of the natural number to four instead of five, and sometimes to a smaller number, is characteristic, but it is by no means uncommon to observe the restoration of the fifth part in both *Fuchsias* and *Oenotheras* under high culture, and, when it occurs at all, it takes place uniformly through all the circles. I have seen various examples in both genera. In the great order *Fabacæ*, the Leguminous plants, a single carpel from abortion through irregularity of the rest of the circle is characteristic, but I have often met with kidney beans with two opposite carpels united by their edges so as to remind us of the maple fruit, and in *Acer Pseudoplatanus*, the Sycamore, I have found, instead of the usual pair of carpels, a complete circle. We are thus forced to admit that the parts deficient in particular structures are absent through abortion, but were rudimentally present in the bud, capable under favourable influences of being developed.

In fact the number five is very common in the exterior circles of Dicotyledonous plants, less so in the gynæcium, though often occurring there also; four is often produced both by pressure and by irregularity, three is occasionally found, and two rather more frequently, whilst in cases of the least amount of development, where the circles are reduced to two, or even one, a single organ in that circle is all that appears. In monocotyledonous plants the number three, and, from additional circles, its multiplies, is somewhat more constant, but abortion or degeneracy of organs from irregularity, is found throughout the Musal and Orchidal alliances and in grasses; and other irregularities of number occur. Our general laws of Floral structure, once understood, leave little difficulty in recognizing the proper explanation of the facts as they fall under our notice.

Having now shortly reviewed those principles which may be regarded as admitted among those botanists who apply themselves to the theoretical relations of the flower and its organs, tracing what is common and accounting for what is varied in the different structures, and having ventured to add one or two suggestions for improving these views or the mode of expressing them, we are prepared to estimate the evidence for any additional principle, where we have to judge whether the phenomena are susceptible of good explanation by the aid of those already established, or really require some new generalization for the correct expression of what occurs, and the perception of its true relations with other facts—and then whether the proposed principle agrees

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with and harmonises all the facts so as to be received as what we call a good explanation of them. The kind of facts which *chorisis* undertakes to explain are cases in which the symmetry of the flower as commonly understood would suggest the expectation of one organ, but we actually find two or more, and these in an unusual degree of proximity; cases in which the multitude of apparently distinct organs produced in close proximity seems inconsistent with the supposition of their belonging to successive circles; those in which a number far exceeding the natural number seems to be found distinctly in one circle, and those in which a number of similar organs are combined at their base in clusters, the number of clusters corresponding to what might have been expected to be the number of organs. All these are represented as being capable of explanation by collateral chorisis or the subdivision laterally of one organ into a number of organs. There is also a different class of facts, such as the occurrence of organs arising on the face of other organs and opposite to them: sometimes of lines of opposite organs, which being supposed inconsistent with other principles of structure, are explained as cases of *transverse chorisis*, or the division of a single organ into folds like the splitting of a card into two or even many similar or related organs. It cannot be denied that the cases to which chorisis has been applied as an explanation are attended with some difficulty, and that some of them are even incapable of plausible explanation by previously established principles. Some of them, however, appear to me quite consistent with those principles, as I shall endeavour to show when examining some alleged examples, and although it cannot reasonably be affirmed that such an operation as chorisis is inconceivable as arising from the nature of the organs of the flower, and it seems even to be sanctioned by some facts, yet I find myself obliged at least to limit its application within much narrower bounds than some able botanists have assigned to it. My reasons will be best given in an examination of the particular cases brought forward at least a sufficient number of them to justify a general opinion on the subject. I shall take the examples given by Dr. Gray, who adopts fully the theory of chorisis in his valuable work, the *Botanical Text Book*, pp. 250-255, having reference also to his remarks in "The genera of the United States Flora, illustrated." Dr. Gray's first example of collateral chorisis, on which he is disposed greatly to rely, is found in the Tetrastemonous stamens of the natural family *Brassicaceæ*. This case I considered at large in a paper read before the Cana-

dian Institute in Feby. 1860, and published in Vol. V. of the "Journal, p. 382, to which I now refer. I accept the quaternary symmetry in Brassicaceae, but consider the two lower stamens as part of an exterior circle of which two glands frequently present represent the other two members. I see no pretence for regarding the two pairs of stamens as each representing one divided organ, and I explained in consistency with my own view all the facts produced. Dr. Gray's second example is found in the androecium of Fumariaceae. This consists apparently of six stamens in two groups of three each, and Dr. Gray regards them as really two organs, each divided into three by collateral chorisis. It is to be observed that the two lateral stamens of each group have one anther each, while the central one has two. This suggested the theory of DeCandolle, supported by Lindley, that there are really two pairs of stamens, but those which were in the direction of the lateral pressure are split into halves, one half of each being pushed close to the stamens of the other pair, so as to place the perfect stamen of each end between two half stamens divided from the other pair. The Brassicaceous monstrosity recorded in which an outer stamen is split so as to resemble two each with a single anther, greatly supports this explanation which is favored also by the separated anthers, one on each side the column from the single stamen of many Orchidaceae and the instances of widely separated anthers with a partially divided filament. On this supposition there may be said to be a chorisis, but it is one of the most intelligible kind as there is no creation of an additional anther or of anything more than is present in the undivided stamen. It must be remembered that as chorisis is assumed to be a division from above, the three stamens in *Dicentra* being often quite distinct below and only coherent in the middle is very unfavorable to, I should almost say absolutely inconsistent with Dr. Gray's theory, and whilst this example is before us it is vain to appeal to the more complete union in other Fumariaceae, as it is an obvious case of coherence.

Dr. Gray's third example is one of those cases which appears to me to justify the admission of the principle of chorisis as occasionally giving us a satisfactory explanation of structures which without it seem incomprehensible. He refers to the three groups of stamens each completely united at their base in *Elodea*: justly observing that though the two outer circles in this flower are pentamerous, the inner ones three in number, the carpels, the three groups of stamens, and the three glands are trimerous so that each group of three connected sta-

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mens represents a single organ. The same is true of the organs seemingly representing abortive clusters of stamens in *Parnassia*, and the observation of Duchatre as to the development of the numerous stamens of *Malvaceae* from small protuberances representing the single stamens of the original circle may be confirmed by any one who will examine with attention half-double *Hollyhocks* in which intermediate states are found between bunches of stamens and unfolded petals.

The close bundles of stamens in *Ricinus* and the fan-like groups in some *Myrtaceae* may be of the same kind. Admitting then, the principle to a certain extent, we need not multiply examples. The difficulty is that, supposing the scattered parts of a vascular bundle which forms the leaf to supply the filaments of a bundle of stamens, we should anticipate the divided expansion giving only one cell to each anther, as is the case in *Malvaceae*, but in other cases referred to we have two-celled anthers resulting from the divided leaf, a real difficulty without doubt, yet not sufficient, perhaps, to overcome the reasons in favour of the theory.

Transverse chorisis is quite a different thing and far more incredible than what has thus far been discussed. The leaf of a *Horse-chestnut*, a *Virginian creeper*, or a *Lupin*, occurs to us as a ready illustration of the possibility at least of collateral chorisis, and it being satisfactorily proved that an ordinary stamen is but a leaf developed under peculiar circumstances, a leaf becoming a group of connected stamens cannot seem entirely opposed to our reason, each portion of the leaf has its own vascular bundle to form the filament and its own cellular expansion to form the anther. But when we are told of that which is but a thin lamella of organized substance, with its two surfaces differently constructed, and its intermediate portion quite distinct from both, splitting in planes parallel with its surface so as from the one to produce a number of similarly expanded organs possessing the same general structure as the undivided organ would have done, we may well exclaim against the extravagance of such an assumption, and we try in vain to think of any thing which appears to justify it. A carpel is but a leaf in a peculiar state of development, and as it advances towards maturity as a fruit, we can often separate in a direction parallel with its surface three portions, the epicarp or outer surface, the mesocarp or vascular and intermediate portion, and the endocarp, the inner lining of the fruit corresponding to the upper surface of the ordinary leaf; but these three parts though often separable in fact,

and always in idea could none of them exist as living parts without the others, they are different portions of one organized substance, and the consideration of the sense in which they are different, only the more impresses us with the impossibility of supposing such elements as would ordinarily produce one leaf, capable of producing, under any stimulus, many leaves standing in parallel planes, each containing all the parts of the one. But it may, perhaps, be thought that there is some other mode of representing this matter not liable to the preliminary objection here offered. Dr. Gray, who probably presents the subject as judiciously and plausibly as any one has done, and whose authority would justly go as far as mere authority ever can, is disposed to treat the question as one of fact, as if he said: it cannot be denied that examples occur of multiplication of organs opposite to one another in the flower which do not admit of explanation by their belonging to successive circles—these facts claim consideration whether we can explain them or not, but when stated, an explanation may be attempted—accordingly he begins by putting aside the theory to which my remarks above directly apply, in the words: “The name *dédoublement* of Duval, which has been translated *deduplication*, literally means *unlining*; the original hypothesis being, that the organs in question *unline*, or tend to separate into two or more layers, each having the same structure. We may employ the word *deduplication*, in the sense of the doubling or multiplication of the number of parts, without receiving this gratuitous hypothesis as to the nature of the process, which at best can well apply only to some special cases. The word *chorisis*, also proposed by Duval, does not involve any such assumption, and is accordingly to be preferred.” He adds, respecting transverse chorisis: “Some examples may be adduced before we essay to explain them.” I am myself disposed, nevertheless, to endeavour to understand and consider the theory proposed, and then try its application to the facts. These facts are certain phenomena in flowers which are, if possible, to be brought under general laws of structure. Is it certain that laws previously known do not apply to them? and if this must be admitted is the hypothesis called transverse chorisis the only possible one, and does it answer fully the requirements of the case? These questions we can only answer when we know what the hypothesis is—what supposition respecting the origin of the parts is adopted. That of Duval is quite intelligible, and in the case of collateral chorisis seems reasonable, applying well to some of

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the cases, and supported by some good analogies. In respect to transverse chorisism, it appears to me inconsistent with what is known of vegetable structure and, as Dr. Gray concedes, unsupported by any analogy. But let us inquire what explanation Dr. Gray himself offers and then we can try his hypothesis by the facts. I regret that the *Journal of Botany* not being within my reach at Toronto, I cannot now recur to the paper to which he refers, but the substance of his own view is that the analogue of the floral parts referred to transverse chorisism is found in the ligule of grasses and the stipules of other plants, he does not think the supposition of axillary organs in the place of buds necessary, although he holds that an axillary bud might be restricted to the development of a single phyton, and thus produce organs in the situation expressed by transverse chorisism. Nothing impossible or antecedently very improbable can be alleged against these suppositions. Some recorded monstrosities even encourage our resort to them, but I cannot perceive either of them to be at all needed in some of the examples appealed to, and it is manifest that neither would afford the smallest assistance in explaining cases of many opposite organs occurring one within another; yet in replying to Dr. Lindley's arguments against chorisism, referring to his forcible appeal to the case of certain varieties of *Camellias* in which the organs of successive circles become opposite, Dr. Gray says, "Now, when in the very same species, two such different modes of arrangement occur, is it not *a priori* more probable that the two arrangements result from different causes and are governed by essentially different laws?" I think not. The same organs are present in both cases, and either a diminution or a small increase in the spiral tendency of growth would change the usual alternation into the occasional oppositeness without any thing occurring at all inconsistent with known facts; but if Dr. Gray would receive the opposite petals of these *Camellias* as an example of transverse chorisism, it is at least one which his own mode of explanation could not possibly reach, and which on any principle yet proposed, must appear most extraordinary. Let us now consider a few examples of transverse chorisism by which we may judge whether there is any need for the name or for any new principle applicable to these cases. "A common case," says Dr. Gray (*Bot. Text Book*, 4th ed. p. 253) "is that of the *crown* or small and mostly two-lobed appendage on the inside of the blade of the petals of *Silene* and of many

other Caryophyllaceous plants. This is more like a case of real *dédoublement* or *unlining*, a partial separation of an inner lamella from the outer, and perhaps may be so viewed." But the close relation of the petal to the stamen, and the many instances of a condition intermediate between the two are well known, and it seems easy and natural enough to regard the crown as an imperfect development of anthers whilst the expansion above it corresponds with the petal-like enlargement of the connective in some stamens, and the claw with the filament. Here then, we need no new principle, and find no real exception to recognised laws. The appendage to the stamen in *Larrea* and other Zygophyllaceae is perhaps as good a case as can be found for the application of the stipule theory which has here not a little plausibility, although when we consider the modifications of development in a single petaloid organ as shown in *Ranunculus* with its petal scales, *Helleborus* with its nectariferous cup; some species of *Lilium* with their protrusions on the surface, and again the cases among the grasses of awns which are the midribs of the glumes or paleæ to which they belong, departing at some distance below the apex, we, perhaps, ought not to consider the appearances as inconsistent with the supposition of one organ developed in an unusual manner. Perhaps the appendages at the base of the anther in *Erica* are quite as strange as if they occurred at the base of the filament, and the stamen growing from the extremity of a petaloid process in *Campanula* not much less anomalous than if it rose from the same lower down, or at the base. Then we have the stamen of *Asclepias* with its extraordinary appendages which is as like the unlining of an organ as anything we are acquainted with, yet undoubtedly is no more than a mode of development of the one modified leaf.

The next example is taken from the genus *Parnassia* with its curious and beautiful appendages [nectaries of Linnæus] opposite to the petals immediately within them, and thence inferred to be derived from them, or, as it were, a part of the same organ. These appendages may be some justification of collateral chorisis though the multiplication of parts is incomplete, but I confess I can find no reason for denying them to be a circle of parts originating distinctly in the torus, although they are placed opposite to the exterior circle. I have given reasons for believing that oppositeness alone is no argument for identity of origin in organs, and if it were, the fertile circle of stamens in *Parnassia* must be accounted only a transverse chorisis of the carpels, as the members

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of these two circles are also opposed to each other. The case of the group of stamens with the petaloid scales behind it in the American Linden seems very closely to resemble that of the clusters of stamens, in that instance coherent, of Malvaceae in a half-double flower; of the latter we have the separate petals partially developed as clusters of stamens, and we observe that they are not flat or merely curved, but nearly funnel shaped or folded round again. Let a small portion assume the leaf-like aspect and the rest subdivide into separated stamens and we have a remarkable instance of collateral chorisis in an organ so curved in figure as to produce the very appearance exhibited.

These examples probably include all the varieties that would afford anything special from which to reason, and further details would be unsuitable in this place. I conclude, 1st, that chorisis or the division of a single organ into two or more similar, or approximately similar ones, is a possible and reasonable supposition, and accounts well for a class of facts which the laws of structure previously established did not properly reach. 2nd, that chorisis does not admit of being divided into two kinds, collateral and transverse; that the latter kind as explained by Dunal, to whom we own the theory, is liable to most serious objections, and is not justified by any facts necessarily implying it, or strictly analogous with it; that the explanation adopted by Dr. Gray takes the case entirely out of the formation of separate organs from a single one; and that oppositeness of parts in adjoining circles is no indication of those parts being of common origin or belonging to a single organ, so that transverse chorisis may be entirely set aside. 3dly. That the ingenious and distinguished authors who have proposed and defended the law of chorisis have been led to apply it in various cases which do not really come under the law, and are better explained on other principles, particularly that there is no chorisis in Brassicaceous flowers, and that a number of organs really derived from several distinct circles may be so pressed together as to form one apparent circle, the parts even being connected by a common expansion derived from the torus, so that a number of crowded parts however regularly set is no proof of chorisis.

With these restrictions I receive chorisis as an additional principle in the structure of flowers, affording us valuable assistance in bringing them all, however varied, within general rules, and manifesting their common relations.